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THE JOURNAL OF PHILOSOPHY

IMPRESSIONS AND APPRAISALS OF ANALYTIC PHILOSOPHY IN EUROPE. II[1]

III

THE *Wiener Kreis* has propagated its doctrines to all parts of the world with far too great a missionary zeal to require more than a perfunctory introduction for an American audience. Within the last three years, however, significant shifts in positions taken have been made by some of its members. Indeed, following the tradition of most radical movements, the threat of a split into a "left" and a "right" wing is on the horizon. Those who base their criticism of the logical positivists on the earlier writings of the group may therefore find themselves to be flogging the wrong horse. It will perhaps obviate some misunderstanding if I first give a brief account of the views once held in common by all the members of the *Wiener Kreis,* and follow it by a sketch, necessarily inadequate, of more recent developments.

A. The *Kreis* originated as a discussion group with Moritz Schlick as its leader, when he was called to the chair of philosophy at the University of Vienna in 1922; but it did not attain self-consciousness as a philosophic "school" until much later. The decisively formative influences on its growth were the positivism of Comte and Mach, and the mode of logical analysis made prominent by Russell. The pragmatic conventionalism of Poincaré and the methodological researches of Duhem also helped mold its present outlook. But these diverse intellectual ingredients were precipitated in a characteristic manner by Wittgenstein's *Tractatus.* For two years the semi-monthly meetings of the *Kreis* were devoted entirely to reading and deciphering its cryptic contents, with the consequence that what was at first an unaffiliated discussion group grew into a militant champion of what it understood to be Wittgenstein's teachings. However, a unanimous and unqualified endorsement of the contents of the *Tractatus* was never given.

A remarkable feature of the *Wiener Kreis* is that for the most part its members are not professional philosophers, and even when they hold chairs of philosophy they have behind them years of solid

[1] Continued from preceding issue, No. 1.

training in some special discipline. Without being aware of it, they have taken seriously Peirce's advice that expert knowledge of some empirical subject-matter ought to be part of a philosopher's equipment. I attended two meetings of the *Kreis*, and was impressed by the fact that the departments of mathematics, physics, law, medicine, and sociology were as well represented in the gathering as were the professional philosophers. Such a catholic interest in problems of logical analysis would be difficult to find elsewhere than in Vienna.[2]

The best known characteristic of the *Wiener Kreis*, both in its earlier and later stages, is its "anti-metaphysical" tendency. Metaphysical propositions are those which, while purporting to be about matters of fact, yet refer to some domain beyond possible experience, so that "metaphysical problems" are not amenable to treatment by the logico-empirical methods cultivated in the various scientific disciplines. Metaphysical propositions are rejected by the Vienna Circle not because they are alleged to be false, but because on the theory of meaning adopted they turn out to be meaningless.

The theory of meaning was, and is, central to the *Wiener Kreis*. According to it a sentence is significant only when the mode of its verification is known; it is a non-sensical combination of words otherwise. Every question, therefore, is in principle capable of having

[2] It would be a mistake to suppose, however, that the *Kreis* is identical with the individuals who meet regularly in Vienna. Some of its adherents, like Philipp Frank, have never been regular frequenters at its sessions; and some, though once physically part of the *Kreis*, are now distributed all over the globe because of political and private reasons. Moreover, there are groups of students, like the one centered around Hans Reichenbach before he found it necessary to leave Berlin, which are in large sympathy and active coöperation with the Vienna Circle even though they have never given explicit assents to its doctrines. Indeed, I have the impression that the *Kreis* as a definite group meeting regularly has seen its best days. For Rudolf Carnap's departure for Prague, Otto Neurath's exile to The Hague, Kurt Goedel's visits to Princeton, Hans Hahn's premature death, have deprived the Circle of some of its most original and strongest members.

Exigencies of space make it impossible to include accounts of the views of several men who, although they are important contributors to analytic philosophy in Central Europe and are in sympathy with the temper of the *Wiener Kreis*, nevertheless do not subscribe to the characteristic doctrines of this group. I have in mind especially Hans Reichenbach, now at Istanbul, and Felix Kaufmann of Vienna. Reichenbach's views are familiar to American students, but Kaufmann is less well known. The contents of the latter's book on the mathematical infinite (*Das Unendliche in der Mathematik*) were thoroughly discussed among the members of the *Kreis*, who at one time shared Kaufmann's semi-intuitionistic views. In the context of a penetrating analysis of the methodological problems of the social sciences Kaufmann indicates his differences from the present tenets of the *Kreis* in his forthcoming book *Methodenlehre der Sozialwissenschaften*.

a determinate answer. It was thus not simply exuberance of wit which prompted the comment which I had from one member of the *Kreis* on Nicolai Hartmann—that every time Hartmann makes an error in analysis one could be certain he will baptize it as another *aporia* or irresolvable antinomy.

The verification so essential for significance must however be more fully explained. According to the original version of the doctrine, only those statements are verifiable which are referable to final, indubitable "facts" given in immediate experience. "Verification" was thus understood to mean complete, "once-and-for-all" decision concerning the truth of statements, so that a necessary consequence of this view is that theories and other general expressions, which are not verifiable in this manner, are strictly speaking without meaning. In fact, theories were rejected as "genuine" propositions, and were interpreted as being simply *rules* for constructing truly significant statements. At this stage in its growth, therefore, the *Wiener Kreis* succumbed completely to the Machian ingredient in its composition. It was committed to a belief in ultimate atomic facts of a sensory kind, to a program of "logically constructing" the world out of elementary data of sense, and to a thoroughgoing solipsism with respect to method—though it was suspected by some that the solipsism extended beyond the confines of methodology.

A careful study of Russell's logical researches on relation numbers as well as of the nature of verification made clear to the *Kreis* that only what is *communicable* can properly be called knowledge, and that only the logical structure of facts and not the qualities experienced in verification can be communicated. Following Wittgenstein, the *Kreis* supposed that the communicating language pictured or mirrored the logical structure of facts, because theoretically every statement was analyzable as a truth-function of atomic statements each of the latter corresponding to an atomic fact. A heroic effort was thereupon undertaken to mitigate the solipsistic features in the theory of meaning. Carnap tried to show how a common, intersubjective language, which is the vehicle for communicating logical structure, may be generated out of a set of mutually exclusive private languages, each statement of which is meaningful only to its asserter and only at the moment when he is verifying it in his own sensory experience. But about the details of this undertaking or about how far it was successful, I shall have nothing further to say.

The first important change in the position outlined above was made by Neurath, who had from the beginning been in opposition to the dogmatic elements in Wittgenstein's teachings. In the interest of precision Carnap had called those sentences which reported the

experiences of the asserter of a proposition, the asserter's *protocols*, and he assumed that the protocols were necessarily atomic and ultimate. Neurath now suggested that the notion of "atomic facts," together with the idea of private, incommunicable experiences, were irrelevant to the procedures of the sciences, and were therefore a metaphysical excresence upon a theory of meaning which took its stand upon the actual practise of science and informed common sense. He therefore argued that protocols are not in fact atomic statements and that there was no reason to suppose they are verifiable once and for all.

Neurath's suggestion, supported by Frank and Karl Popper, was accepted and generalized by Carnap, and finally incorporated into a comprehensive theory concerning the objective and method of philosophic analysis. It marks the parting of the road between two groups within the *Wiener Kreis*. Schlick and Waismann still maintain, apparently in agreement with Wittgenstein, that there are indubitable "perceptual propositions" upon which all our matter-of-fact knowledge rests. Schlick admits that protocols in the sense that Carnap and Neurath employ the term do not possess the finality which he requires for the foundation of empirical knowledge. But he insists, nevertheless, that there are non-hypothetical "propositions" won in observation. These "propositions" (*Konstatierungen*) occur not at the *beginning* of scientific inquiry, but are the ultimate elements in terms of which verification is made. They are really not communicable, they can not even be recorded in speech or writing, for it is essential to them that apprehension of their meaning coincide with the apprehension of their truth—and this can occur only at the knife-edge instant of observation. Schlick and Waismann both accept Wittgenstein's more recent views on grammatical analysis. Nevertheless, they seem to me to be committed, perhaps like him, once more to a doctrine of atomic propositions, and to a confounding of having knowledge with having sensations. Schlick at one time made much of the difference between *Erkennen* and *Erleben*, and showed at great length the difficulties which opened up when an ineffable seizure of objects was demanded of knowledge. At a crucial point in his present views he seems to me to have abandoned one of the most valuable of his earlier insights.

B. Like most other members of the *Wiener Kreis*, Carnap's thinking was controlled for some years by the leading ideas in the *Tractatus*. However, since his assumption of the chair of philosophy at the *Deutsche Universität* in Prague, he has been developing a point of view incompatible with much of the earlier one of Wittgenstein, even though as a consequence of independent evolution their present doctrines still coincide at many points. For Carnap, too, has come

to the conclusion that the subject-matter of philosophy consists of statements which are elements in some more or less precisely formulated language, and that the task of philosophy lies in clarifying the meaning of sentences by exhibiting and stating the syntactical (grammatical) rules which control their usage in a specified language. A consistent working out of this idea, combined with great fertility in attacking technical problems in the methodology of the formal sciences, characterizes his recent work. I wish to report upon three intimately connected phases of Carnap's present interests, to the extent at least that a non-technical account of some of them is possible: his theory of meaning, his theory of syntax, and the theory known as "physicalism."

(1) Carnap still formally subscribes to the theory of meaning according to which the meaning of a proposition lies in the mode of its verification. However, verification and what can verify is no longer conceived by him as narrowly as it once was. For the conviction that the domain of logic is coincident with the domain of *discourse* or *language,* has persuaded him that verification as a *logical* process can not consist in comparing statements with "facts" lying outside of discourse; verification is the process of comparing statements requiring evidence with *other statements,* the protocol-statements.

In the light of this reformulation of what verification is, the dispute about the existence of and necessity for "atomic facts" now takes on a different color. In the first place the question requires to be restated so as to be about atomic *statements* or *sentences,* (i.e., sentences which contain only words referring directly to elementary data of sense), and not about "facts." And in the second place, the controversy is declared to be capable of an empirical decision, since one may resort to inspection to decide whether atomic sentences indeed occur in the protocols of the sciences.

However, Carnap believes that the dispute about atomic facts is in general not one about the truth of propositions concerning empirical subject-matter; if it were it would have long since been settled by the usual methods of empirical study. For the dispute does not concern the forms of propositions *actually* admitted into the protocols of the sciences, but rather what *should* be the form of protocol statements. Now Carnap maintains that this is not decidable on purely empirical grounds, because the type of protocol statement is fixed by a more or less explicit *convention.* One may stipulate that the protocols should all have an "atomic" form; or that they be sentences reporting various sorts of configurations in the observational field; or that they refer to physical objects in a physical field, e.g., sentences of the form "The moon is now on the horizon." Carnap

thinks that we are free to make any of these demands, and that each of these demands can be satisfied by a consistent theory of protocols. Indeed, I think it is legitimate to interpret Carnap to say that the form of protocol sentences admitted varies with specific contexts, and that *in general* the forms must be left unspecified: in some situations protocols will have the form of general propositions or theories which are taken for granted in those contexts; in other situations the protocols will be singular propositions, and so on. The sole requirement for protocols upon which I think Carnap would insist, is that at *some* point in the development of an empirical science protocols will have to be introduced which are *primitive* protocols—i.e., sentences which contain words referring to what is directly observed by the asserter of a proposition.

One immediate consequence of this formulation of what verification is, is that there is no theoretically necessary ultimate terminus to the process of verification. The dogmatism of indubitable atomic facts is replaced by the view that every protocol is theoretically capable of further verification. From a broad point of view there is no difference in kind between the verification of a theory and the verification of a singular proposition. Both are hypotheses, which commit their asserters to an indefinite series of further propositions, most of which refer to the future, so that a complete, "once and for all" verification no longer is possible. The simple—far too simple—positivism of earlier days has thus undergone a radical transformation. The Machian strain in the *Wiener Kreis* has progressively been conquered by the insight of Duhem that it is always an indefinitely demarcated *system* of propositions, and never an isolated single one, which is put to the test in any observation.

It follows that not only is the *form* of protocols a matter to be settled by convention and practical needs; *which* propositions are included among the protocols is also regarded as the consequence of a more or less explicit stipulation. For on the one hand, since no sentences are significant in isolation and every sentence is an element in a far-flung language system, every sentence has an indefinite number of logical consequences; as was pointed out above, there is no theoretically final verification of any sentence. On the other hand, *some* sentences are in fact accepted as protocols, as "true" and relatively final. It is claimed therefore that this acceptance of certain propositions as "fundamental" is the outcome of what, as a matter of strict logic, is a convention or resolution. Indubitability or certainty is thus a practical, not a logical matter, conditioned by psychological and sociological factors: the protocols we do in fact employ are a function of the manner in which science is cultivated in our own historical epoch.

Carnap's wing in the Vienna Circle believes that not only is this revised theory of meaning and verification more faithful than the original one to the actual procedure of science, but that the annoying solipsism of the earlier version is made completely innocuous. Since verification consists in the *logical* comparison of a sentence or its consequences with sentences in the protocol class of sentences, there are no "private facts" required which would make dubious the very possibility of communication. For while the meaning of a proposition still consists in the mode of its verification, the mode of verification is defined by the syntactical principles of a given language governing the comparison of one set of sentences with another set.

It may perhaps seem as if the escape from solipsism has been paid for by abandoning the empiricism so prominent in the earlier *Wiener Kreis*. That, however, is not the price which Carnap and others think it necessary to offer. For it is still essential to their views that observation words like "see" and "touch" must occur in various places in the protocol sentences. They do not deny, therefore, that sensory observatioin plays a fundamental rôle in empirical inquiry. What they insist on is that, since *logical* comparison can be made only between sentences, in logical comparison there are no "facts" to be reckoned with which lie either within the private consciousness of the investigator or which are brute elements in the flux of events. A complicated process of observation of "facts" must, of course, take place; but the study of this process belongs to psychology or general biology, and not to logic. Logical comparison and therefore verification can be made only with what is the result of observation—not with what may precede observation or with the process of observation itself; and the result of significant observation is always a proposition.

However, the distinction between a proposition which is "metaphysical" and so meaningless, and a proposition which is significant, can no longer be made in practise so easily as before. For the meaning of a sentence is established in terms of the syntax of the language of which it is an element, and since the syntax of languages actually employed is a complex matter, more may be involved in determining the meaning of a proposition than at first appears. We must, however, examine Carnap's views about syntax.

(2) Building upon the earlier meta-mathematical researches of Hilbert and the Polish logicians, Carnap holds that every language, whether it is the common language of every-day speech or a specially constructed "symbolic" system, consists of a number of elements which constitute its vocabulary, e.g., words, and a set of operative rules which constitute its syntax or grammar. The syntactical rules

are of two kinds. One set, the "formation rules," states how the elements of the vocabulary may be combined to form statements, and which thus constitutes the conditions for significance in the language. The other set, the "transformation rules," specify how statements may be replaced by or translated into other statements; among the transformation rules will be found the principles of inference.

In the case of "artificial" languages we are free to select both the vocabulary and the syntactical rules, subject to the condition that the latter be consistent with one another; in such languages the principles of inference will clearly be stipulations concerning the manner we are to operate upon statements constructed out of elements of the vocabulary. In the case of languages actually employed in some historical epoch, the contents of the vocabulary must be obtained by an empirical survey, and the syntactical rules will be descriptive of the manner in which the given language is employed. For the latter group of languages, syntactical rules are not free stipulations in the sense that at some time or other men contracted to employ them rather than other rules; nevertheless, they are conventional in the sense that it is in accordance with them that the language is employed and that alterations may be made in them to suit the convenience of its users. Moreover, in the case of a "found" language the syntactical rules may not in fact be consistent with one another, and changes may or may not be proposed to bring them into such consistency. In the case of the scientific languages in our historical epoch, however, consistency is an actual ideal pursued.

It is possible both in the case of "artificial" and "found" languages, Carnap believes, to *state* the syntactical rules formally, without reference to what the elements of the vocabulary may designate —that is, without introducing what is usually called the "meaning" of the terms. When they are so stated, the syntax of the "formalized" language is concerned with the structures of possible serial orders between the elements of that language, and may itself be developed deductively from certain initial definitions. On the other hand, formal or "pure" syntax may be cultivated from the point of view of its applications, in which case it becomes descriptive of the syntactical properties of some given piece of discourse.

There is no room in this general account for the detailed achievements of this program of research, for which Carnap's recent publications must be consulted. I do want to mention, however, one conclusion Carnap has obtained, which stands in violent contradiction to an important doctrine of Wittgenstein's *Tractatus*. According to that book, since language "mirrors" or "shows" the logical

structure of "facts," but can not *state* that structure, philosophy has no doctrinal content and must be construed simply as an activity. Hence the *Tractatus,* which does try to state important propositions about logical structure, must be adjudged to be full of "nonsense" statements. This paradox, however, turns out to be only an apparent one. For Carnap has been able to show that given a rich enough language it is always possible to state without contradiction the syntax (logic) of the language in that very language. It follows that neither an infinite hierarchy of languages nor an appeal to a "mystical," non-discursive "beholding" is required in logical studies; and it also follows that statements about syntactical or logical relations can be regarded as the specific doctrinal content of a philosophy which is not "metaphysical."

In consequence of the above conception of logic as syntax, it is an error, according to Carnap, to regard the principles of logic either as descriptive of the way we think or as ontological " laws of being." Principles of logic are either descriptive of the way in which sentences are translatable into others (e.g., the principle of *ponendo ponens*), or definitory of the meanings of certain terms (e.g., the principle of non-contradiction). By their means we define what we understand by a coherent, consistent system of language, and we explicate what it is we mean by certain statements within the context of the specified language. This view may easily be dismissed by some as a species of nominalism; if so, it is the "nominalism" professed in different terminology by such "realists" as Duns Scotus and Charles Peirce. Carnap, together with the other members of the *Wiener Kreis,* does *not* maintain that statements like "Freely falling bodies move in a vacuum so that the distance traversed is proportional to the square of the time" is one about *words* or other signs; *that* statement reports, or purports to report, relations which characters of things bear to one another. There are statements, however, which are not about behaving things, but about the explicit or implicit stipulations we employ in *representing* such things. Logical "properties" are properties of terms in discourse, of signs in a system, they refer to our "second intentions" and not to the things represented.

We must therefore distinguish, according to Carnap, three types of statements. The first type consists of "real-object" sentences, e.g., "That man weighs 200 pounds," or the inverse square law, which are about extra-linguistic objects and the relations between them. Such statements are the product of empirical inquiry. The second type consists of statements about syntactical relations between sentences, e.g., "All men are mortal and No men are immortal are equipollent." The third type, important because they are the

source of much confusion, consists of "pseudo-object" (or quasi-syntactical) sentences; these have the appearance of real-object statements, although in fact they are syntactical ones. For example, the sentence "The fact that a man can live forever is impossible" is a pseudo-object statement; it appears to predicate the property "impossibility" of an entity, a "fact." That it is indeed a syntactical statement will be evident if we render it as follows: "The sentence 'A man can live forever' contradicts acknowledged laws of biology." When we translate it in this manner we find that "impossibility" is not the name of a property characterizing an entity called "fact," but refers to the logical relation of one sentence to others. In Carnap's terminology, a pseudo-object sentence is employed in the "material mode of speech" when it appears to refer to some extra-linguistic objects; it is translated into a correct "formal mode of speech" when it is clearly exhibited as a syntactical one.[3] The distinction between material and formal modes of speech obviously holds only for pseudo-object sentences.

As a consequence of his recognizing just these three major types of propositions, Carnap maintains that the doctrinal content of philosophy consists in syntactical statements. Since real-object sentences belong to the domain of the special sciences which investigate their truth and falsity by empirical procedures, and since every other significant statement is in fact if not in form a syntactical one, any sentence which claims to be about matters of fact without being subject to the canons of scientific inquiry is meaningless. Carnap believes that traditional metaphysical propositions fall into the class of meaningless statements, in so far as these propositions coincide neither with statements capable of some degree of empirical verification nor with statements about syntactical relations. All alleged examples of *a priori* material propositions illustrate statements of this sort. Metaphysical, and therefore meaningless, problems are generated, he thinks, by failing to recognize pseudo-object statements for what they are. Carnap freely admits that the material mode of speech with respect to them is often innocuous and convenient in practise; nevertheless, it frequently invites serious confusion. Thus the familiar questions concerning the nature of space, time, substance, causality, determinism, are adjudged to be pseudo-problems, because those who ask them look for the determinations of substantial entities, instead of inquiring into the syntactical properties of the sentences in which these terms are employed in a

[3] Let O_1 be some non-linguistic object and O_2 its designation. If, whenever a property P_1 is ascribed to O_1 a syntactical property P_2 belongs to O_2, P_2 is called by Carnap a "parallel syntactical property" to P_1. Any statement which ascribes the property P_1 to O_1 is then a pseudo-object sentence.

specified language. But before illustrating Carnap's technique in eliminating pseudo-problems, I wish to describe briefly the doctrine known as "physicalism," of which Carnap, Frank, and Neurath are the outstanding exponents.

(3) Physicalism is at present largely a program of activity, and even the statement of its objectives is still in the process of formulation and refinement. It represents another point of division between the two wings in the *Wiener Kreis,* although as far as I know neither Schlick nor Waismann have stated explicitly their disagreements with it.

Since physicalism states its objectives in terms of "the language of physics," one must first win some degree of understanding of the latter before discussing the former. Since, however, both the vocabulary and the syntax of this language are still in the process of being made explicit,[4] a crude description of it is all that can be given at present. The vocabulary of the language of physics consists primarily of terms denoting spatio-temporal positions, spatio-temporal regions, and quantitative determinations of physical states associated with either. The syntactical rules of the language include those usually recognized as the principles of logic, as well as the rules of procedure employed in mathematics, probability, and the contemporary science of physics. It will, of course, also contain specifications concerning the form of its protocol statements, and the conditions under which general statements may be accepted or rejected on the basis of the accepted protocols. Consequently, the language of physics may be roughly defined as a language containing statements asserting quantitative relations between spatio-temporal objects, and which is continuous with and a refinement upon the language of every-day practice.

So-called qualitative statements, involving qualitative specification of spatio-temporal objects, are not excluded from this language, for such statements are frequently more useful for purposes of every-day communication than the quantitative ones, and in any case are more compact than the latter. However, it is the ideal of the language of physics to introduce measurement of one kind or another whenever possible, and so to specify quantitative correlates for qualitative distinctions; for the introduction of numerical coefficients makes possible the formulation of the laws of nature independently of specific sensory qualities and so permits the application of the principle of substitutibility to data of sense.[5]

[4] Carnap plans to devote his next book to this subject.

[5] It is essential, I believe, to distinguish "the language of physics" from the language actually employed to-day in the special science of physics. For the intent of the exponents of physicalism is to specify the former in such a

Assuming, then, that the nature of "the language of physics" can be adequately specified, physicalism asserts that every significant statement in any domain of investigation can be translated into it without loss of *logical* content, i.e., roughly, without loss of anything which can be communicated. A language for which this is possible is said to be a universal language; a language which can be translated into a universal language but which is not rich enough to have every significant statement translated into it is called a partial language, e.g., the language of psychological experience. Physicalism claims that the language of physics is the sole universal language with which we are at present familiar. This thesis is offered as a proposition in descriptive syntax.

The bearings of physicalism upon contemporary discussions in the philosophy of science can be made clear in connection with a subsidiary doctrine professed by the Carnap wing of the Vienna Circle—the thesis of *Einheitswissenschaft* or "unity of all science." It has been frequently asserted that the hypothetico-deductive method employed in the natural sciences (briefly referred to as "scientific method") is not applicable to human affairs, so that a distinct logic must be employed in historical researches. German philosophers in particular have made a sharp division between *Natur* and *Geisteswissenschaften,* which are said to have in common neither subject-matter, aim, nor method. Not subject-matter, it is said, because the former sciences are concerned with repeatable patterns of behavior, and the latter with the historically unique, or individual; not aim, for the natural sciences are essentially pragmatic and seek to control the flux of events, while the *Geisteswissenschaften* wish to understand and appreciate; and not method, for while the former employ scientific method, the latter are dependent upon non-discursive insights into the nature of concrete historical situations. On the basis of the distinction between the two kinds of sciences, there would then be two different kinds of knowledge with the consequence that the propositions of the *Geisteswissenschaften* could not even be *stated* in the language of the natural sciences.

way, that even though the science of physics undergoes serious modifications and consequently modifies its terminology and the laws of nature which it accepts, nevertheless the special science will at all times be employing "the language of physics." Thus the latter is to be understood in such a way that it is comprehensive enough to permit statements in it of modes of specifying relations between objects which are not at present employed in the science of physics. Moreover, "the language of physics" is to allow statements reporting sensory-experience and the routines and "physical facts" of daily life to be included in it. To put it crudely, "the language of physics" is a language in which the reference of statements to the procedures implicit in our having knowledge of any subject-matter receives the maximum of explicitness.

Physicalism is a protest against this bifurcation. It asserts in so many words that the subject-matter for all empirical inquiry is the world of spatio-temporal events. It recognizes that events and objects may be of different degrees of complexity, but claims that apart from differences in special techniques required to explore their structures a common logical method is applicable in every department of knowledge. Hence it maintains that every proposition in the *Geisteswissenschaften,* which is significant in the sense specified by the theory of meaning already sketched, is translatable into the universal language of physics. There is therefore only one kind of knowledge, only one science (though it may be departmentalized for reasons of convenience), and only one kind of general subject-matter—spatio-temporal events and objects differently related to one another.

It would be a serious error to suppose that physicalism asserts matter, atoms, electrons (or any other fashionable entity of current physics), to be exclusive realities. It does not subscribe to this kind of reductive thesis. Nor does it claim that every discoverable law in biology, psychology, and human history is deducible from the laws of physics as these are stated at present. What it does maintain is that whatever is *knowable* in *any* field of inquiry is *structure;*[6] and that however we may state our knowledge, the propositions obtained are *translatable* without loss of *meaning* (i.e., without neglecting some phase of the structures discriminated) into the language of physics—a language which is an extension and refinement of the language of practical daily life.

I wish to supplement this account of Carnap's philosophy—already too long in terms of pages written but much too brief from the point of view of an adequate report of its details and subtleties—by three examples of syntactical analyses he has made.

1. The relation, or lack of it, between mind and body is a characteristic theme of modern philosophy. Carnap tries to show that a "two-stuff" theory of reality is not necessary to explain the alleged duality of mind and body, and that the content of the duality lies in the fact that different syntactical rules govern the occurrence of these words. The question is discussed by Carnap in connection with the problem of our knowledge of other minds.

Consider the statement "Smith is angry." In terms of Carnap's theory of meaning, the meaning of this statement must consist in the mode of its verification. In this case the mode of verification will be the formulation of certain observation-sentences about Smith's body, including the statements Smith himself makes. "Smith is angry" means, in brief, "Smith's *body* is behaving in

[6] This statement is, of course, analytic of the term "knowledge."

certain ways." However, it may be objected that to Smith himself *his* anger will mean something else, because he verifies it by introspective-statements, even though this can not be done by anyone who isn't Smith. Hence, it is concluded, to drop back to the material mode of speech, the physical configurations we observe are distinct and different in kind from the psychical qualities which Smith experiences and reports. Consequently, it is further claimed, the relation of the physical to the psychical, whether one is the cause of the other, or whether they are non-interacting parallel phenomena, is a legitimate problem.

Carnap's reply runs as follows: *If* we distinguish anger as psychical quality from anger as physical behavior, then "anger" (as psychical) occurs in sentences (belonging to the language of psychological experience) whose syntax is different from the sentences (belonging to the language of physics) which contain "anger" (as physical behavior). Dualists are in the right in so far as they maintain that the word "anger" (physical) can not be employed in accordance with the syntax governing the use of "anger" (psychical). Nevertheless, in virtue of the empirical law "A person who is angry (psychical) is also angry (physical)," the two sentences "Smith is angry (physical)" and "I (Smith) am angry (psychical)" are *equipollent*. Their logical content, in virtue of the empirical law of nature, is therefore the same, and they have the same *verifiable* meaning, both for us and for Smith himself. And consequently the puzzle about the relation between two sorts of entities depends upon our using reduplicating terms like "anger" (psychical) and "anger" (physical). Genuine problems, e.g., the conditions and consequences of being angry, of course still remain to be settled by empirical inquiry. And it must be noted that Carnap does not *deny* the experiencing of a quality of anger by Smith. But this experienced quality can not be conveyed by the statement, "I am angry" made by Smith, however much we may associate with it images of qualities we ourselves experience. Hence Carnap's analysis is intended to reveal what it is we *know* in statements about other minds, and to distinguish such knowledge from non-cognitive experiences.

2. English philosophers have discussed with much vigor whether all the properties of a thing are external or internal to its nature, and some like Bradley have concluded that all properties are internal, while others like Russell have held that only some traits have this characteristic. Carnap suggests that the controversy and its difficulties are consequences of using the material mode of speech in connection with pseudo-object sentences. Suppose we say that a property P is internal to an object a, if it is impossible that a

should be without it, and external in other cases. It will follow, for example, that Ariston who is the father of Plato is internally related to Plato. On the other hand, it is not an internal property of the father of Plato that he be a landlord. Nevertheless, it is an internal property of the owner of an estate to own it, so that it would seem that after all the father of Plato, who is a landlord, possesses the internal property of owning a certain estate. The source of the puzzle and apparent contradiction become evident immediately when we recognize that these statements are pseudo-object ones, so that the characteristic of being an internal property is a syntactical one within a specified language. Hence what we mean by "A landlord possesses the internal property of owning an estate" is correctly rendered by the syntactical sentence "The sentence 'A landlord owns an estate' is analytic." (It is analytic in virtue of the definition of "landlord.") On the other hand, since "The father of Plato owns an estate" is not analytic, we can not obtain the above absurd results when we express the relevant propositions in the formal mode. And in general, the question about internal relations is to be stated in the form "Is every sentence analytic?" and is definitely settled with the exhibition of one sentence which is not analytic in the language.

3. Nowhere has Carnap shown the power of syntactical analysis so clearly as in his discussions of the antinomies in the foundations of mathematics and of the theory of proof. These matters are unfortunately too technical to report here. However, I wish to indicate briefly his approach to the *Grundlagenstreit*. As is well known, there are at present three prominent theories concerning the nature of pure mathematics: the "formalist" view sponsored by Hilbert, the "constructivist" philosophy of Brouwer, and the logistic thesis of Frege and Russell. Each standpoint carries with it important consequences for mathematics, for entire branches of the subject would require to be liquidated if the fortunes of this dialectic warfare were favorable to one of the contestants rather than to the others. Nevertheless, although the controversy is now of long standing, it seems at present no nearer settlement than when it began.

Carnap proposes to act as a mediator in this dispute. He has therefore undertaken to state as clearly as he can the syntax of the languages employed by each school. He has shown that certain symbolic operations which are permitted by the syntactical rules of the formalists are excluded in the linguistic system of Brouwer. Consequently, the latter's claim that certain theorems of pure mathematics are without sense is adjudged to be correct *if* Brouwer maintains that *within* the language he permits himself to use those theorems can not be stated. On the other hand, since the syntactical

rules employed by Hilbert are more inclusive than those of the "constructionists," the mooted formulae are perfectly legitimate *within* the system of the formalists. The issues between Hilbert and Brouwer, so far as these are statable explicitly, are thus settled by Carnap by invoking what he calls "The Principle of Tolerance":— Hilbert and Brouwer employ different languages subject to different syntactical rules. Each of these languages, so far as we can tell, is consistent. But the *choice* of a consistent language is dictated by neither fact nor logic; the choice rests upon a convention. Hence since logical analysis discovers simply the syntactical rules which characterize a symbolic system, logic can not legislate *which* language *should* be employed. Logic is dictatorial *within* a language having determinate syntactical rules; it is neutral and tolerant with respect to the *choice* of a language system, a choice which is a function of practical and esthetic needs.

Concerning the dispute between the formalists and logisticians, Carnap proceeds in a similar manner: Hilbert is quite correct in maintaining against Frege that pure mathematics can be developed and cultivated altogether formally, without needing to know the "meaning" of its signs. However, Frege and his followers are also in the right in asserting that on the formalist view the manner in which the formulae of pure mathematics can be made to *apply* is not specified; for in Hilbert's mathematics only the syntax of mathematics as an autonomous discipline is investigated, and the syntactical rules which relate his "meaningless signs" with the expressions in the language of common-day life and the positive sciences are ignored. In so far, therefore, as the logistic view demands for the logical foundations of mathematics the syntax of the *universal language* of which mathematical calculi are only a part, the Frege-Russell dissatisfaction with the formalists is justified.

I do not know whether in this account of Carnap's present views I have been able to convey anything of the single-minded attachment to the business of clarifying ideas which characterizes the man. I chanced to hear Carnap take part in a discussion before knowing who the speaker was, and was immediately impressed by the gentle force of a luminous mind. I listened to him subsequently, in classroom and seminar as well as in private, and thought I understood what Emerson meant when he described character as a reserved force acting directly by presence, apparently without means. As a lecturer I found him admirably lucid, as the leader of his seminar incisive and yet sympathetic. In private discussions he was a patient listener to my problems, clarifying them not only by suggesting solutions, but answering many by reformulating the difficul-

ties. He is one of the rare people with whom one does not have to agree in order to be understood by him. I had a sense of immeasurable thoroughness and of untouched intellectual resources behind almost everything he said, and even when I was left unconvinced there was no denying the bold consistency with which he marshalled his arguments. I still do not know to what extent or at precisely what points I differ from the doctrines he set forth. And in what follows I am conscious of voicing doubts and hesitations rather than definite disagreements, and of pointing to sins of omission rather than commission in his present views.

(*a*) The rejection of atomic facts as the necessary basis for common-sense and scientific knowledge seems to me to be an important improvement upon the theory of meaning once held in common by the neo-positivists. I am not certain, however, that I understand the full import of regarding the acceptance of protocols as being grounded on convention. It is surely using the word "convention" in an extraordinary way, to apply it both to the selection of a special form of theory and to the acceptance of the propositions which serve as evidence for it. It seems clear to me that there are some propositions whose truth we can not help admitting whatever may be the causal and historical explanations for our admissions. I fail to note any element of practical choice in admitting some propositions as true, even though there is always the genuine possibility that we may be in error in so doing. Indeed, it seems to me that syntactical analysis itself shows that "convention" in connection with the acceptance of primitive protocols, "convention" in connection with the selection of a general theory, and "convention" in connection with deciding upon such matters as which side of the road is to be employed by traffic, have three distinct meanings, and that to identify them is indeed a Pickwickian use of language. I suspect that every proposition is said by Carnap to be conventionally accepted when it is not demonstrated logically; hence, since such demonstration is never possible for material propositions, every proposition about matters of fact is based on a "stipulation." But this seems to me to be lumping together things which ought to be distinguished. Since Carnap does not believe that science is simply human caprice, and he does think there are better reasons for acting upon predictions made on the basis of scientific method than by a clairvoyant, it is essential that he specify more carefully than he has yet done the procedures involved in arriving at the protocols.

Indeed, I think Carnap has thrown out the baby with the bath in excluding from philosophy the study of the process of observation through which protocols are obtained. For while it is legitimate to departmentalize research and to define logic as the study of syntax,

a coherent account of meaning and knowledge is sacrificed by not considering the more inclusive situation in which thinking has its origin, development, and terminus. By taking note of this larger context, I think Carnap would be able to do more than make a bare gesture toward the esthetic and practical needs which control the conventions he thinks he is making. It would be a pity if syntactical analysis were to stop short of the analysis of material categories.

(*b*) The Principle of Tolerance with respect to choice of languages invites related difficulties. In so far as a "language" is viewed as a formal calculus, as a consistent game played according to certain rules, I can see how a choice between different "languages" so considered is arbitrary, and how the Principle of Tolerance would apply. Nevertheless, the intellectual function of language is to communicate, and on Carnap's view to communicate structure. It seems to me, therefore, that the language "chosen" must be adequate to convey the structures discriminated, so that the Principle of Tolerance can not be a correct principle. Nor do I see that the point at issue can be dismissed by declaring that the language employed is selected for its "usefulness"; for in this country, at least, the long debate concerning the relative priority of the "useful" and the "true" has indicated that nothing is accomplished by merely substituting one word for another.

In particular, I do not think that the contending parties in the mathematical *Grundlagenstreit* can finally settle their differences on the basis of the principle. I imagine they could all assent to Carnap's conclusion that they are employing languages with different syntactical rules. Nevertheless, they could still claim that the language employed is intended to communicate the *result* of analyzing a certain subject-matter, so that the inclusion or rejection of certain syntactical rules is a function of the structures of subject-matter explored and the methods used in exploring it. This seems to be tacitly admitted by Carnap in what he has to say about the short-comings of Hilbert's theory and about the advantages of the more comprehensive program of Frege and Russell. For he there indicates that an adequate philosophy of mathematics must show the connection between mathematics as a formal calculus and the language system of common-day experience. And I take it that this means, to speak loosely by relapsing into the material mode of speech, that the symbolic operations of pure mathematics are to be shown to be continuous with the symbolic operations of every-day speech and the structure conveyed by the former to be continuous with the structure and operations of and within empirical subject-matter. If this were not the case, Carnap's theory of meaning and

the doctrine of physicalism would themselves be completely arbitrary and of no importance to philosophers. Carnap's preoccupation in recent years has been the syntax of artificial languages, in which the syntactical rules are explicit and can be constructed by new resolutions on how to employ symbols. I wonder to what extent this preoccupation has led him to neglect the fact that the clarification of terms in discourse actually employed is not won by simply legislating arbitrarily new rules of syntax, but that the shock of experience, failure to communicate, and incongruities in overt behavior, are the clues we must follow in developing a more "useful" language. Since Carnap himself allows room in his conception of philosophy for the study of *descriptive* syntax (which is an empirical science requiring observation), there is no reason why a little more room could not be created for the study of the empirical, operational reference of linguistic expressions. For the "meaning" of expressions is defined not simply by the *formal* syntactical rules of the language containing them, but also by the various occasions and operations when these expressions are *used.*

(*c*) A further point of difficulty, but which I can barely mention, is Carnap's distinction between transformation rules of a language, which he calls "logical," and those, like Maxwell's equations, which he calls "physical." If the Principle of Tolerance were really a valid principle, why should it not be possible to introduce into a language any kind of "physical" rule of syntax, or to develop a language in which what are now called "physical" syntactical rules would function as the "logical" one do? But what sort of language would it be in which these "physical" rules would be tautological? Clearly there is some limiting condition upon the freedom to choose languages, a constraint which itself is not selected by the users of a language.

(*d*) Carnap's detection of pseudo-object sentences and his distinction between the material and formal modes of speech concerning them seem to me first-rate intellectual contributions. But I am not happy over some of the examples he offers as illustrating this class of sentences. For example "Smith visited Africa" is a real-object sentence; but "This book treats of Africa" is offered as a pseudo-object sentence in the material mode of speech, which is correctly rendered by "This book contains the word 'Africa.'" For as Carnap goes on to explain, "This book treats of Africa" does not assert anything about Africa: "It is not a quality of Africa to be treated of in that book, because one might know everything about Africa and nevertheless know nothing about that book. On the other hand, it is really a quality of Africa to be visited by Smith."[7]

[7] *Philosophy and Logical Syntax,* p. 65.

It seems to me that at this and several other points Carnap is not faithful to his principle that the meaning of a sentence is given by the syntax of the language which contains it. Is the syntax of either English or German so definite that we can at once say whether a sentence in question is a real-object sentence or not? From what prior definition of "Africa" does it follow, for example, that being written about is not a "real" property of Africa? Is Carnap not committing Whitehead's fallacy of simple location in supposing that Africa and her properties are "really" contained within her coast line? Common language is much more flexible and vague than this analysis of Carnap's would imply, and molded more definitely by the exigencies of and the structures revealed in practise than he would perhaps admit. This and several other illustrations he offers may of course have been unfortunately selected; certainly many of the terms employed in physics and the social sciences, especially law, would at once become unintelligible and generate pseudo-problems if they were taken to denote such atomized segments of experience as Carnap seems to think "Africa" denotes. But in any case, it is in accordance with Carnap's general procedure that I suggest that whether a sentence is a pseudo-object one or not is "relative with respect to language."

(*e*) The doctrine of physicalism, as I understand it, does not follow logically from the present version of Carnap's theory of meaning. Since it is still a program and the outlines of the language of physics have only been sketched thus far, it is not easy to evaluate its merits. The arguments for it, as stated at present, are not altogether conclusive, as can be seen in the above sketch of Carnap's discussion of our knowledge of other minds. Carnap maintains that sentences containing terms of psychology are equipollent with sentences containing terms belonging exclusively to the language of physics. But this equipollence is admittedly not a "logical" one. It holds only in virtue of certain empirical laws, which correlate psychic with physical states. To which language, however, do the statements of these empirical laws belong? They can not belong exclusively to the language of physics, for it is essential that one of the terms correlated by them should belong to the language of psychology. And the fact seems fatal to such an argument for physicalism, although I do not say or think it is fatal to physicalism itself. It seems to me that on this occasion Carnap has been temporarily persuaded by the neat terminology of his formal researches to maintain what is so essential for him to deny—that Smith's feeling of pain is a case of knowledge.

(*f*) Perhaps the least satisfactory part of Carnap's views is his analysis of the propositions of ethics and esthetics. Ethical norms

are declared to be imperatives and not statements capable of truth or falsity, while propositions of esthetics and traditional metaphysics are regarded as being merely expressive with no theoretical content. But it is surely not the case that ethical propositions are necessarily either assertions about some transcendental autonomous realm of values or statements simply about human feelings. They can be conceived as hypotheses concerning ways of organizing or reorganizing the human scene in the interests of the well-being of its members. Ethical statements would still be empirical and would refer to the want and capacities of living beings; but they would be more than records of the private feelings of those who make them.

Similarly, the sharp distinction Carnap makes between utterances and actions which are merely "expressive" of feelings and those which convey information, seems to me unsatisfactory. A lyrical poem may not contribute to our knowledge about meteorological facts; but must we suppose therefore that its only function is to enlighten us about the poet's feeling? There is the conceivable alternative that poetry and other art objects call our attention to patterns and distributions of qualities, even if it is the case that the occurrence of such qualities is conditioned by the presence of certain biological and social organisms. But in any case one may readily admit that esthetic experience is not knowledge, without subscribing to the analysis of art objects as being simply registers of past feelings.

IV

One consequence of the last European war is the appearance of contributions to scholarship in languages not generally cultivated as part of the apparatus of philosophers. Since the re-establishment of Poland, Polish thinkers have published many of their important researches in their national language, about some of which only fugitive hints can be obtained from summaries in French and German. My visit to Warsaw and Lwów was therefore a profitable experience for me. Nevertheless, oral instruction about the current work of the Polish logicians is not the most satisfactory way of learning about their achievements, and I confess to gaping lacunae in my understanding of them. Moreover, since the logical researches both at Warsaw and Lwów are extraordinarily specialized and technical, in this general report I can give only a superficial account of what is being done.

I found Poland to be an interesting and remarkable country philosophically. A nominalistic naturalism, dominated by the logico-analytic method, seems to be part of the orthodoxy in most Polish universities. As in Russellized Cambridge, concern with special prob-

lems rather than the manufacture of vast systems is the daily fare of both students and professors. I have been informed that the "scientific" tradition in philosophy, colored by a materialistic *Weltanschauung,* harks back to the late eighteenth century. Interest in logical studies in Poland, though naturally stimulated by the work of Whitehead and Russell, has not been created by them. Some of those holding chairs of philosophy have been students of Frege, and his writings as well as those of Schroeder were studied carefully long before the *Principia* was born. Through Twardowski, now professor emeritus at Lwów, Brentano's version of Aristotelian realism received a wide hearing. All in all, if the men I met are representative of Polish philosophers, there is less romantic *Schwärmerei* among them than in any other country with whose philosophic activity I am familiar.[8]

The Warsaw school of logic is oriented around the interests of Professors Jan Lukasiewicz and Stanislaw Lesniewski, their most brilliant and gifted pupil, Afred Tarski, and the young mathematician Adolf Lindenbaum. Their primary concern is with mathematical logic, and they have set up for themselves an ideal of formal rigor only seldom envisaged elsewhere. The logic of induction, probability, and allied topics in scientific method interest them, as far as I could learn, only in so far as the theories concerning them have been adequately formalized into deductive systems. Warsaw logicians have recognized long ago that we must distinguish clearly between propositions *in* a language and statements *about* them. On the analogy of Hilbert's theory of meta-mathematics they have therefore created a special branch of research, meta-logic, which contain statements *about* logical calculi and the procedures required to operate them. It is in this field that Tarski has most clearly exhibited his talents; he developed a deductive theory of syntax or metalogic several years before Carnap's publications on this topic.

Lukasiewicz's chief recent interest has been the calculus of propositions. His many important researches include the discovery, prior to and independently of Bernays, of methods for proving the independence and completeness of axioms for such a calculus. Nevertheless, the history of logic has held his attention for many years, although his publications in this field are still regretably few, and has suggested to him a way of extending the traditional calculus; it is this extension or generalization which has won for him international recognition. A close study of Aristotle's writings, especially of his discussion of propositions referring to the future, led Lukasiewicz to formulate a calculus which allows for the possibility

[8] There is an account of Polish analytical philosophy, together with a good bibliography, by Professor Ajdukiewicz in *Erkenntnis* Bd. 5, Heft 2/3.

of propositions being "indeterminate." This was the origin of the three-valued calculus of propositions, which was later generalized into one having *n*-values; in these new calculi various modal relations between propositions are to be permissible values of the implicitly defined constants. The significance of these strange systems is not beyond dispute, and Lukasiewicz is himself less confident of their interpretation than are some of the bolder spirits who find the problems of probability illuminated by them. Neither Lesniewski nor Tarski has been able to discover in them anything of philosophic importance. However, these new calculi have become fresh subject-matter for metalogical researches by Lukasiewicz and his students. In particular, the study of the relations between the two methods of generating *n*-valued calculi of propositions (i.e., the postulational and the matrix approaches), has led to interesting and fruitful theorems in meta-logic.

Lesniewski, though little known outside of Poland, is held in an esteem in Warsaw comparable to that with which Frege is now universally regarded. For more than a generation and with incomparable rigor he has been constructing a system of logic adequate to carry through the logistic program for the foundations of mathematics. What I have read and understood in his extraordinarily difficult German publications makes the treatment of similar themes in *Principia Mathematica* appear as lacking in rigor. He is especially careful to specify minutely the *directives* or rules of operation required to work the various calculi. His logical system, built up in three divisions, has the merit of keeping closer to the common or Aristotelian interpretation of certain logical constants than is usually the case in symbolic systems. The third division, called by Lesniewski *Mereologic,* develops the formal apparatus, *à la* Whitehead, for dealing with events. But my information about the *Mereologie* is especially meager, since there is no account of it in an international language which I could consult to supplement the hints I obtained about its contents. Unlike most other professors of the logico-analytic method and like Moore in this respect, Lesniewski rejects a purely formal approach to logic and mathematics and disagrees with the current conventionalistic interpretation of scientific systems. According to him, logic is not a mechanical play with "meaningless" marks, for logical structure has its roots in the natural world and reveals something about it.[9]

[9] Interest in less formal aspects of logical theory is not completely lacking at Warsaw. Under the stimulus of Professor Kotarbinski—an influential pupil of Twardowski, a nominalistic materialist, and one of the few men in Warsaw academic life vitally and courageously interested in social theory and practice—a number of younger men and women have been cultivating various parts of the methodology of science. I cite, in grateful remembrance of the exciting

Those who are not particularly engrossed by the minutiae of mathematical logic will find Lwów a more congenial place than the capital of Poland. Professor Leon Chwistek holds the chair of philosophy of mathematics at the University of Lwów, and while he is necessarily preoccupied with the details of the logical apparatus required for analyzing mathematics, his interest is not merely technical. His work is motivated by the desire to squeeze out from the foundations of mathematics the mythological platonism and theology which still haunt many a worker in this field. He himself believes he is reconstructing mathematics on a "nominalistic" basis, and in a sense this characterization is just. For he regards the subject-matter of pure mathematics to be the serial properties of signs, and not the relations between disembodied essences. However, too much must not be made of the label, for "nominalism" in the sense of rejecting every vestige of structure from natural subject-matter is professed by none of the men upon whom I am reporting. Perhaps a more appropriate name for the sort of thing Chwistek is doing is formal semantics, the general character of which is similar to the researches in formal syntax by Carnap and others. Chwistek's reconstruction of mathematics on the basis he has selected is still incomplete. It may be of interest to mention his extralogical activities: He is a painter of note in his own country as well as a literary critic whose judgment carries weight. His concern with social questions is vital and contagious, and he exhibits rare courage in expressing political and social sympathies in a country where open acknowledgment of them has serious consequences.

Though a great admirer and former pupil of Lesniewski, Professor Kasimir Ajdukiewicz has been developing a general interpretation of the logical researches of recent years which places him in a philosophic camp different from that of his teacher. He has worked out what he calls a "radical conventionalism," according to which our knowledge of the world, as expressed in some language, is controlled by categories of meaning which characterize that language, so that the expression of our knowledge is "conventional" down to the most elementary perceptual propositions we affirm. In general outline, the position is similar to that of Professor C. I. Lewis in this country, so that an exposition of its details, of its strength and weaknesses, may be omitted here. Nevertheless the approach and the arguments of Ajdukiewicz differ in important ways

evenings spent in talk high and deep, the work of Dr. Janina Hosiasson on probability and induction, and of Dr. Aleksander Wundheiler and Mr. Edward Poznański on general methodology in physics. I also record the pleasurable surprise I experienced in finding that Norman R. Campbell's valuable "Physics, The Elements," whose readers are rare both in this country and in England, was known and admired by this group of Warsaw students.

from those of Lewis. While both take their departure from a theory of meaning, Ajdukiewicz obtains his conclusions through an analysis of language. He recognizes that for defining the meaning of expressions there is required not only their formal syntax, but also certain "meaning-giving" rules of usage for these expressions in specified contexts. Distinguishing carefully between axiomatic, deductive, and empirical "meaning-giving" rules, he argues that even in the case of empirical statements controlled by the latter it is never the case that an isolated proposition can be affirmed, but always a proposition whose meaning is defined by a complicated set of categories. Since, however, this set of categories depends on the choice of a language, and since a language may be replaced by another or altered in essential ways, the proposition affirmed is conventionally grounded. This radical conventionalism of Ajdukiewicz, formulated with the care and precision characteristic of the Polish logicians, is thus another philosophically significant outcome of the continental interest in semantic analysis.

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BOOK REVIEWS

L'opinion selon Aristote. L.-M. RÉGIS. (Publications de l'Institut d'Études Médiévales d'Ottawa.) Paris: J. Vrin. Ottawa: Institut d'Études Médiévales. 1935. 284 pp.

The author traces the history of the concept of opinion from Xenophanes through Plato, and then defines it in terms of Aristotle's usage and shows its place in the Aristotelian system. He admits (p. 56) that Aristotle never examined the concept in itself and did not appear interested enough in it to define its nature. Yet he holds that this seemingly disdainful attitude has led scholars to overlook its importance in the Aristotelian philosophy. What the present work attempts, therefore, is to gather together the references to opinion in Aristotle's writings, systematize them as Aristotle himself never did, and then place the systematized concept in the psychological, logical, and practical doctrines. In a sense, the author is thus going beyond Aristotle; but readers of the book, even if they disagree with some of the conclusions drawn from the evidence, will be inclined to feel that Aristotle ought to have systematized his thought on this point and to be grateful to the present author for performing the task.

The author shows that opinion, in Aristotle's use of the term, is not merely an unstable or confused cognitive state, but that it is

also a kind of intellection directed toward what is probable; and he comes to the conclusion, after some debate, that it embraces both τὸ συμβεβηκός (the accidental) and τὸ ὡς ἐπὶ τὸ πολύ (what is for the most part or generally true). It is then "a judgment whose object is the contingent being of things in their concrete, individual existence (immediate opinion) or the necessary being that is apprehended not in itself but in its accidents (mediate opinion) . . ." (p. 108). This definition is carried through the three regions of experience which Aristotle called, respectively, theoretical, practical, and poetic.

The main significance of the work lies first in the historical study, and second in bringing together such widely scattered Aristotelian ideas as the physical concept of the accidental, the logical concept of dialectical reasoning, and the ethical concept of what is generally accepted. It is thus a very significant contribution to Aristotelian studies.

R. S.

The Mind of Paul. IRWIN EDMAN. (Studies in Religion and Culture, Schermerhorn Lecture, III.) New York: Henry Holt and Co. 1935. 187 pp. $1.75.

Professor Edman in his *Mind of Paul* has made a notable contribution to the study of religion and shown how effective that study can be when direct and realistic. Indeed his contribution is so intimately connected with his approach that the significance of the latter may be overlooked. He takes the conversion of Paul as an initial mystical experience not to be explained in terms of heredity and environment, but as a fact which explains what Paul did with his heredity and within his environment, and which explains also, within limits, the effect of Paul on the historical spread and development of Christianity. Such an approach is in marked contrast to the conventional methods of evolutionary, comparative, and psychological studies of religion. It is giving to Paul the position in history of a transforming and effective religious force. It is not explaining him by history, but using him to explain a history. Obviously Paul was a Jew and used his heritage. What else had he to use? Obviously, too, he was a man of his age. But, as Professor Edman says in a sentence which sums up the whole book, "Paul was a man of his age, as well as a mystic of all ages" (p. 171). One may ask with Professor Edman, What would the Christian mission have been without the conversion of Paul which touches and quickens something universal in religious experience? Paul's transformation of his conversion into a philosophy of history is not sufficiently developed. Professor Edman frees Paul from the entanglements of the theology later built upon the "Epistles," but it seems

clear that Paul, in the light of his conversion, saw the history of his race as the significant preliminary of the history of mankind.

F. J. E. W.

Religion and Science. BERTRAND RUSSELL. New York: Henry Holt and Company. 1935. 271 pp. $2.00.

There are many who will hail with satisfaction the return of the author of *A Free Man's Worship* to the subject-matter which provoked that famous essay and its companions in *Mysticism and Logic.* But the intellectual atmosphere round about the conflict between science and religion has changed considerably since 1910. "On the whole," admits Lord Russell, "the fight is languid as compared with what it was. The newer creeds of Communism and Fascism are the inheritors of theological bigotry; and perhaps, in some deep region of the unconscious, bishops and professors feel themselves jointly interested in the maintenance of the *status quo.*" It is therefore as a historian who sees science as the complete victor over "organized ignorance" that the author approaches the old battlefields of Copernicus and Mr. Scopes of Tennessee. His opening chapters, as he freely acknowledges, hardly do more than bring Andrew D. White's *A History of the Warfare of Science with Theology in Christendom* up to date in tabloid form. In spite of Lane Cooper's researches, he re-tells the hoary anecdote of Galileo and the weight-dropping from the Leaning Tower of Pisa.

With the chapter on "Soul and Body," followed by others on "Determinism," "Mysticism," "Cosmic Purpose," and "Science and Ethics," Lord Russell enters upon topics of current philosophical discussion with his accustomed *verve* and irony. Many of his remarks seem to have been inspired by a series of twelve B.B.C. radio talks on Science and Religion ("as the State wishes them to appear," he charges) delivered in 1930. In his rebuttal, Lord Russell shows that he has not lost his keen eye for the joints in the armor of the official champions of orthodoxy and their attending "reconcilers." Always a master of the aphorism, he still uses that weapon with telling force:—"From a scientific point of view, we can make no distinction between the man who eats little and sees heaven and the man who drinks much and sees snakes"; "The harm that theology has done is not to *create* cruel impulses, but to give them the sanction of what professes to be a lofty ethic"; "Emergent evolutionists, having become persuaded that God did not create the world, are content to say that the world is creating God." There is an interesting prophecy on page 214: "I incline—though with hesitation—to the view that there will ultimately be a science embracing

both physics and psychology, though distinct from either as at present developed."

H. A. L.

OTHER NEW BOOKS AND JOURNALS

Kierkegaard, Sören: Le concept d'angoisse. Traduit du danois par P.-H. Tisseau. Introduction de Jean Wahl. Paris: Félix Alcan. 1935. 240 pp.

THE PHILOSOPHICAL REVIEW. Vol. XLV, 1. Philosophy in France, 1934–1935: *André Lelande.* Contemporary German Philosophy: *Arthur Liebert.* Plato's Theory of Forms: *A. K. Rogers.*

INTERNATIONAL JOURNAL OF ETHICS. Vol. XLVI, No. 2. Putting Politics in its Place: *C. E. Merriam.* The Ethics of Fascism: *T. V. Smith.* Naturalistic Standards: *J. R. Reid.* The Writer's Dilemma: A Case History and a Critique: *L. M. Rosenblatt.* "Caveat Emptor": *F. C. Sharp* and *P. G. Fox.* The Moral Conception of Nature in Indian Philosophy: *D. M. Datta.* Discussion—Pragmatism and Social Action: *F. H. Knight.*

REVUE PHILOSOPHIQUE. 60 Année, Nos. 11 et 12. Athènes et Jérusalem: *L. Chestov.* Une découverte scientifique: La durée bergsonienne: *R. Mourgue.* Conscience scientifique et désintéressement: *L. Basso.* Revues Critiques—Psychologie du langage: *A. Gurwitsch.* La sensation, selon M. Salzi: *E. Wolff.*

NOTES AND NEWS

The newly-elected officers of the Eastern Division of the American Philosophical Association are: President, Edgar S. Brightman; Vice-President, George H. Sabine; Secretary-Treasurer, Arthur E. Murphy; new members of the Executive Committee, Raphael Demos and Horace L. Friess.

The newly-elected officers of the Pacific Division of the American Philosophical Association are: President, H. G. Townsend; Vice-President, H. L. Searles; Secretary-Treasurer, Everett W. Hall; new members of the Executive Committee, Everett J. Nelson and W. R. Dennes.